

a living model placed against the wall, and coloured to a depth of 6 inches all around it. The native chiefs who accompanied the Resident said that the remains of the Hill-Papuans had formerly been deposited here, but were now interred with Mahomedan rites; there were indications, however, that some *prahus* had been recently lodged on the platform.

Though the most astonishing part of Mr. Leon's report, viz. the difficulty of drawing the figures on the rock at a considerable height above the sea, is not encountered by Mr. van Braam Morris's experience, it is not proved that the latter explored exactly the same place as Mr. Leon. But just this point (the considerable rising of the islands) is most plainly stated with regard to the Ke Islands by Messrs. Alliol, Mol, van Slooten, Meijboom, and Deijl, of H.N.M.S. *Samarang*, which at the time of their visit lay off Tual (5° 37' 30" S. lat. 132° 44' E. lat.), island of Little Ke. These gentlemen were invited by Mr. Langen, the head of the English settlement there, to visit with him the north-western part of the island; after having steamed for three-quarters of an hour they dropped anchor *vis-à-vis* Kalumit, a village at the base of a hill, about 200 metres high. They went to the top to see there some idols situated in a small settlement. I pass over this part of the narrative, and take it up after they had descended from the edge of the rock, where they had found a burial-place belonging to the kampong, which is on the top. A tolerably well-made flight of ironwood steps allowed the visitors to descend easily; after about half an hour's walk they came to the "necropolis."

On the rock near it they discovered representations in red of various figures—human hands, with the fingers spread out; imitations of human heads; a fight between men armed with *klewangs* (= cutlass), and other figures which they took to be representations of the evil spirits, outlines of ships, &c. Though the heads were rudely drawn, the hands, which were fewer in number, were remarkably well done. The place where the drawings are seem to be quite inaccessible to human beings. In the rock are also caverns which are rather difficult to approach. In one of them two gongs and some pieces of bamboo were found; at the entry fragments of broken glass had been spread, probably to prevent visitors from entering. It must be mentioned that the rock, from the base to the top, was covered with sea-shells. Attention is repeatedly drawn in the report to the circumstance that it seems incomprehensible how the pictures could have been drawn on the rock, which overhangs.

The natives connect the rock-pictures with the burial-place on the top of the cliff. Near the edge of the steep descent stand two houses, which serve as mortuaries, one being close to the dwellings of the natives, which are surrounded with a stone wall. These two houses are built of ironwood; on the roofs there are two pieces of wood, the one in the shape of a prow, the other in the shape of a keel. On the latter are two figures, a dog and a bird; a stick bearing a piece of white cloth is stuck into the bird's body. The walls are 4 and 3 metres, and in the shorter, which faces the sea, there are two doors, through which the coffin is carried; inside this hut they saw two coffins with fruits and a bottle of oil which had been left for the spirits.

The natives, who called themselves Hindoos or heathens, a name which of course has no ethnographical significance, but is merely used to distinguish them from their Mahomedan neighbours, said that when a dead body was placed in the hut the spirit was conducted by the bird or the dog on the roof to the caverns where it is to abide. In token of its arrival the animal draws a figure on the rock. The natives who accompanied the explorers durst not set foot within the caves.

It was also said that the bird and the dog were merely symbols. The soul of the deceased, on leaving the body, flies as a bird through the air or runs as a dog over the earth, till it reaches the abodes of the spirits—the caverns—unseen by living men. Every soul that reaches this haven draws a figure on the face of the cliff. In explanation of the contest between human beings and evil spirits in the pictures, they said that the latter try to prevent the souls from reaching the eternal dwellings; but they cannot hinder those who have led good and honest lives, though those who have done wickedly are carried off by the evil spirits.

The officers, judging from the many articles in gold and silver which were found in the caverns, concluded that they must formerly have been used by pirates as places of refuge and for hiding their stores, and that they were then nearer to the level of the water. On this view the drawings on the rocks would answer a double purpose: they would keep the superstitious from approaching the caves, and would also act as a landmark

for the pirates themselves when returning from sea, and indicate to them the places where their treasure was hidden.

Without hazarding any opinion upon such incomplete accounts, I wish to state, merely by way of summary—

(1) That Mr. Leon's evidence, combined with that of the officers of the *Samarang*, would seem to indicate that the surfaces of certain islands in McCluer inlet and of the Ke group have been considerably elevated.

(2) That the rise has probably taken place at no distant date, but how long since cannot be determined until (perhaps) after close scientific examination.

(3) That Mr. Morris's explorations, taken in conjunction with the foregoing, suggest that the elevation is not a general one, but, though observed at distant points, is limited to certain islands of different groups, or even to particular sides of them.

Stuttgart, March 18

EMIL METZGER

Mr. Lowne on the Morphology of Insects' Eyes

PROF. LANKESTER appears to me to be fighting too much under cover. First he sends his lieutenant into the field, and then he appears himself, in the guise of an independent ally. But inasmuch as he has virtually accused the officers of the Linnean Society of having published a paper unworthy of a place in the *Transactions* of the Society, I feel fully justified in bringing him out into the open.

The anxiety expressed by Prof. Lankester on behalf of the Fellows of the Linnean Society, as to whether my paper was refused by the Royal Society, is manifestly insincere: he knows as well as I do, that the paper was virtually refused by the Royal Society. As Prof. Lankester is taking undue advantage of the secrecy which attaches to the office of referee, I shall state the facts with which I am personally acquainted, and I doubt not these will place the whole matter in a very different light from that which Prof. Lankester has endeavoured to shed upon it.

It is evident Prof. Lankester wishes to make it appear that the rejection of my paper by the Royal Society confirms his strictures and those of his lieutenant, and enables him safely to attack the Linnean Society under cover of the Royal. Now, I believe that every one who was concerned in the publication of my paper knew perfectly well that Prof. Lankester was the first referee to whom it was submitted by the Royal Society. Prof. Lankester wrote to me himself, and stated that the paper had been so referred. Although I then felt sure of its rejection, I should not have had any reason to complain, if the rules of the Royal Society had been carried out, and the paper had been submitted to a second, entirely independent referee. Prof. Huxley, in his opening address to the Royal Society on his election as President, stated that every paper was considered by two entirely independent referees. Now, in my case the second referee was Prof. Schäfer: I do not think it right to refer a paper to two colleagues intimately associated in the same school; and I am sure that no consultation should take place between the referees pending their decision. Yet Prof. Schäfer heard Prof. Lankester's adverse opinions expressed in my presence before he came to any decision himself—at any rate before making any report; and he confessed to me that he had no special knowledge of the literature of the subject on which he was called upon to give an opinion.

Under the circumstances I feel justified in stating that, if the Royal Society had rejected my paper, it would have been a rejection by Prof. Lankester; and I feel sure that an independent referee would have done exactly what was subsequently done on behalf of the Linnean Society.

Prof. Schäfer recommended me to withdraw my paper; I petitioned the Council of the Royal Society to allow me to do so, and the paper was returned to me. If this be a rejection, my paper was rejected.

I then presented it to the Linnean Society, and in so doing I told the Zoological Secretary everything that had happened. The result was that, after some delay, the paper was ordered to be printed in the *Linnean Transactions*.

I could hardly have conceived it possible that any scientific man could have descended to such a device in confirmation of his own views as to pretend that the Royal Society had formed an independent judgment under such circumstances. Prof. Lankester has succeeded admirably in rendering himself impersonal as a representative of the Royal Society—a feat which

would have no doubt incited his just indignation if it had been performed by his friend "Sludge," of spiritualistic celebrity.

I cannot help remarking on the coolness of Prof. Lankester's assertion, that my views are "undeniably based upon a mistaken interpretation of defective preparations." Prof. Lankester evidently thinks his opinion final—but he is bold to say it is "undeniable."

My sections have been seen and approved of by a great number of competent histologists and zoologists, and, although some of them are not so pretty as those prepared by the paraffin method which Prof. Lankester extols, they certainly show a great deal more. The paraffin method is well known to me, and I have examined a great number of slides prepared by it. I have possessed a series of sections so made in the Cambridge laboratory by an excellent histologist, and have rejected them as worthless: they show nothing but the connective tissue framework. Nerve fibres and nerve end organs are alike destroyed.

The whole question of the effect of reagents on the tissues is a wide one. The paraffin process destroys much which remains in the cocoa-butter process, first devised by Prof. Schäfer. I esteem this process far superior to that now used in the laboratory at Cambridge, and by Prof. Lankester and his assistants. I should not fear to place my specimens side by side with Prof. Lankester's before an unbiassed histologist; and I am content to wait the decision of future observers upon my work. New views are met with little favour by those who are committed to old ones, and, whether I am right or wrong, I expect no justice from a critic who shows such determined bias as Prof. Lankester.

BENJAMIN T. LOWNE

If Prof. Lankester imagines that he has any complaint to make against the Council of the Linnean Society for having published Mr. Lowne's paper, I must decline to consider the subject with him in your columns. He is himself a Fellow of the Society, and the anniversary meeting of the Society is due next month. If he then thinks it wise to ask any questions upon the subject, I shall be in my place and most happy to answer them.

GEORGE J. ROMANES,
Zool. Sec. L. S.

How Thought presents itself among the Phenomena of Nature

IN your issue of the 12th inst. the Duke of Argyll asks, "Is there any difference in this respect between molar and molecular motion?" namely, as regards the persuasion which most men entertain that where there is motion there must be some "thing" to move. The answer to this question appears to be the very direct one that there is the following fundamental difference between molar motions and *some* molecular motions, and that it intimately concerns that belief. *All molar motions are secondary motions, i.e. they consist in the drifting from place to place of underlying motions (and, indeed, in the case of those motions which human beings can perceive even with the utmost aid of the microscope, they consist in the drifting from place to place of vast accumulations of such underlying motions), while, in contrast to this, there are some molecular motions which are primary—i.e. which have no other motions underlying them, and which do not consist in the drifting from place to place of more subtle motions.*

His Grace correctly expresses the common opinion in the following words—that "an atom¹ is only conceivable as an ultimate particle of matter." Now the term "particle of matter" in this statement needs to be scrutinised. As commonly understood, it means something minute which we should be able to feel or see or perceive by some of our senses were it not for the bluntness of those senses; and this, as science shows, means that

¹ The Duke of Argyll here employs the word "atom" in its etymological sense; and it is scarcely necessary to point out that the term when so used signifies a different thing from any of the sixty-seven complex bodies known to chemists as chemical atoms, which have intricate internal motions as betrayed to us by the spectroscope, and of which the molecules of compound bodies are known to be made up. The chemical "atom" could not under any view be spoken of as an *ultimate* particle of matter.

I understand the Duke of Argyll to propose these words as a description (not of anything the existence of which has been ascertained by experimental science, but) of that substance, matter, or thing the conception of which he and most other men believe to be the "inseparable concomitant" of the conception of motion, but for the existence of which in external nature no other evidence is forthcoming than this supposed law of human minds.

Now, even if the supposed law were a law from which we could not free ourselves, it might reasonably be maintained that it proves nothing about external existence; but in truth it is not a law, but only a widely prevalent habit of mind, as is *demonstrated* by the fact that the study of nature has extricated some minds from it.

certain specific motions are present, viz. motions of those particular kinds which are competent, indirectly and through a long chain of intermediate steps, to finally occasion visual, tactual, or some other sensation in our minds. The statement, accordingly, as commonly understood, *really* amounts to this—that no motion can be present unless certain underlying motions are also present!

But to the uninstructed apprehension the statement has quite a different meaning, a much fuller one, and one which lies outside the domain of motion. Before they have made very careful investigation, men do not know that there is no green colour in grass or hardness in a rock. They are unaware that what is really going on in the grass is not a state of greenness, but vast myriads of motions,¹ each of which is repeated about as often every second as there are seconds in thirty millions of years, which motions in the grass occasion undulatory motions around of a like rapidity, some of which occur within our eyes, and, acting upon some compound or compounds in the black pigment which lies behind the retina, produce there an effect (probably a fugitive photographic effect consisting in some chemical change of one or more of three compounds in the pigment). This change, whatever it is, excites the optic nerve to make a stir within the brain, and *it is this last motion* (which we may safely say is utterly unlike the external phenomenon, though uniformly resulting from it through the steps enumerated above), which is what determines the perception of green in our minds. Similarly, when the vast accumulation of molecular motions which is called my finger approaches that other accumulation of motions which is called a rock, these motions act on each other, and my finger is compressed upon certain nerves, exciting them to produce those motions within my brain which, though quite unlike the motions outside, are the motions that are really accompanied by the sensation of hardness. But by uninstructed minds the colour of the grass and the hardness of the rock are confidently believed to be external phenomena, and not even phenomena of motion at all, but absolutely stationary phenomena in external Nature.

Finally, we must never forget that beliefs in the human mind, whether they be pure or mixed up with errors, can neither control nor even exercise any influence whatever upon what is really taking place in external Nature, which is the object of our investigation. What is really going on in Nature is to be ascertained, so far as it can be ascertained at all, not by projecting human beliefs into external existence, but by applying whatever modicum of dry light we can win from the slow but gradually encroaching progress of scientific discovery. And the necessity for this caution is intensified where we find, as in the present instance, that the belief has resulted from the way our brains and the brains of our ancestors have grown, under the influence of an experience of motion which has been so one-sided that it has never extended to primary motions at all, nor even to any but very coarse forms of secondary motion, omitting, along with many others, all those motions, whether primary or secondary, that occasion most of our sense-perceptions; and all this, combined with suppositions about other phenomena in which these phenomena have been quite misunderstood. Scientific scrutiny, so far as it has penetrated, finds motion throughout external Nature—motions everywhere, motions underlying every phenomenon, however different from motions some of them may seem to common apprehension; and no *scientific* investigation has as yet detected anything but motions. This is the positive side of the inquiry; and its negative side is that it would be manifestly illegitimate to draw an inference about what really exists outside us from the habits of thought which have been engendered in most human minds by a narrow and one-sided experience mixed up with palpable errors. *We, therefore, are not in a position to allege that we know of anything existing in the outer world but motions and relations between motions.*

The abstract of my Royal Institution discourse, which you were so good as to publish, only attempted to give a bare statement of the successive steps of the argument with which it deals, and I fear it is too condensed for clearness; but, as I am myself persuaded that the argument is sound, I hope that your correspondent will find that a fuller account of it which I am preparing will put all its essential parts in a sufficiently distinct light.

Dublin, March 20

G. JOHNSTONE STONEY

¹ The relations which the parts of motion can have to one another or to other motions are all numerical or space and time relations. Motions may be numerous, few, simultaneous, successive, straight, curved, flat, tortuous, swift, slow, periodic, continuous, linear, or pervading a volume; but they cannot be green motions or hard motions.